sea-level. Furthermore, according to the theory of driven cyclones, the progressive motion of the cyclone is supposed to be determined by the "prevailing west component" of the upper currents. At Blue Hill the mean westerly component of the upper current is 35 metres per second at 9000 metres, about 17 metres per second at 4000 metres, and 1 metre per second at 200 metres (*Harvard Observatory Annals*, 1890, vol. xl. p. 447). It is natural to suppose that a driven whirl, in such conditions, would be rapidly toppled over and destroyed. Yet storms persist for days. If, however, a driven whirl did persist in such conditions, its axis if tilted at all would, according to all analogy, be tilted in the direction of progressive motion. Yet the direct observations with kites at Blue Hill, and the observations of clouds by Ley in England, prove that the axis of the cyclone is tilted backward. Moreover, it is reasonable to suppose that the air in the rear of a driven whirl would partake of the progressive motion of the whirl, and this, added to the in-draught, would make the wind velocity in the rear of the whirl very much greater than that of the winds in front, yet such is not generally found to be the case. For these reasons I think the observations do not favour the theory of driven cyclones.

The theory of cyclones with which the observations in temperate latitudes seem best to agree, is the theory which supposes the cyclone to result from contrast of temperature in a horizontal direction. This I have called the *convection theory*. In this theory there are two types of cyclone. The warm-centre cyclone of the lower atmosphere, and the cold-centre cyclone of the upper atmosphere. The best type of the cold-centre cyclone is the polar cyclone; but there also exist in the upper air in temperate latitudes small travelling cyclones or *hemi*-cyclones of the same nature. Horizontal contrasts of temperature are most marked in winter, hence the theory explains why cyclones are most violent in winter. The origin of the horizontal contrasts of temperature is not shown by observation. They probably arise by the interchange of air between higher and lower latitudes. A body of air moving from the equator toward high latitudes would come into a region where it would be nearly surrounded by colder air, and the conditions would favour the production of a warm-centre cyclone. A body of air moving toward the equator would produce conditions favourable to a cold-centre cyclone. H. H. CLAYTON.

Blue Hill Meteorological Observatory, March 30.

Rock-structures in the Isle of Man and in South Tyrol.

IF the intercrossing of two separate systems of folding be the essential condition in the complicated rock-structures so ably worked out by Mrs. M. M. Ogilvie Gordon in South Tyrol, I scarcely think the parallelism with the conditions in the Manx *Carboniferous* rocks can be so close as Mrs. Gordon suggests in her recent letter (NATURE, March 22, p. 490).

So far as I have been able to judge, the disturbances in the Carboniferous volcanic rocks of the Isle of Man were the result of a movement which was single both as regards direction and time. It is true that this conclusion was reached in 1897, before Mrs. Gordon had taught us the importance of torsion-structure in areas of disturbance; but I re-examined the sections last autumn, after having studied Mrs. Gordon's paper, without finding any reason to alter my former opinion on this point. The interpretation given in my recent paper (*Q.J.G.S.* vol. lvi. p. 11) is therefore in all respects the same as that published in brief in the official Summary of Progress of the Geological Survey for 1897 (pp. 110-112).

It seems necessary, also, to call attention to the small scale of the structures in question in the Manx Carboniferous rocks. Their most striking feature is their sudden local development in a limited tract where the strata are rendered by diverse lithological composition peculiarly susceptible to differential displacement. Under such conditions, it appears that even a small degree of lateral movement may be so focussed as to cause great disturbance at certain places without much disturbance of adjacent tracts. The post-Carboniferous movement in the Isle of Man can scarcely have been even approximately of the magnitude of the disturbances in South Tyrol described by Mrs. Gordon.

It was in pre-Carboniferous times that the Manx region underwent earth-movements of really grand intensity; and Mrs. Gordon may have had this fact in mind in referring to the subject. In the Older Palæozoic (probably Cambrian) slate-rocks of the island, "crush-conglomerate" has been developed on a very extensive scale by differential shearing, as described by Prof. W. W. Watts and myself five years ago (*Q.J.G.S.* vol. li. p. 563). These rocks, moreover, show evidence of successive epochs of disturbance, varying slightly in direction but apparently all pre Carboniferous. The production of the "crush-conglomerate" appears to have occurred during only one of these stages. It is not improbable that an observer acquainted with the "torsion-structures" of the Dolomites might be able to find parallel phenomena among the highly complicated pressure-structures in the Manx Slates; but I think that a sharp distinction should be drawn between these structures and those of the Carboniferous rocks of the island. G. W. LAMPLUGH.

Tonbridge, April 8.

Electric Light Wires and Dust.

I BELIEVE that the collection of dust upon electric light wires and fittings is generally attributed to air currents, due to thermal causes, the same thing occurring, to some extent, with hot-water pipes. Recent experience has, however, convinced me that in the case of electric light conductors, electrostatic attraction is really the chief factor, particularly where the supply is at 200 volts from the street mains. In my office here I have several electric light cords strung across the ceiling. They are all exactly similar and under the same conditions, except that some of them have the switch in the negative and some in the positive conductor. The former gather dust to an extraordinary degree, and now, after a few months' use, have become quite an eyesore. The latter are practically as clean as when first put up. As is well known, the negative conductor of a street supply tends always to earth itself, and, as a matter of fact, in my case I find that the negative of my supply from the Westminster Co. is almost at earth potential. The positive, on the other hand, is nearly 200 volts above the potential of the earth. In this lies obviously the cause of the phenomenon. The wires which have the switch in the negative are nearly at 200 volts potential above the earth whenever the switch is off, while those which have the switch in the positive are at zero potential in these circumstances. Of course, when the switches are on, all the cords are under similar conditions, one conductor in each being nearly at 200 volts above earth, and the other at about earth potential. No doubt it is when the switch is off, in the case where it is in the negative conductor, that the accumulation of dust takes place. Having regard to the comparative lowness of the 200 volts potential, from an electrostatic point of view, the rate at which the dust accumulates on the cords is most surprising, and this is my reason for thinking it worth while bringing the matter to notice. A. A. C. SWINTON.

66 Victoria Street, Westminster, April 23.

ON THE SIZE AT WHICH HEAT MOVEMENTS ARE MANIFESTED IN MATTER.

IN the molecular theory of heat it is assumed that the motions of atoms and molecules are the motions upon which the phenomena of temperature depend. These motions are assumed to be very irregular, and the apparent uniformity of structure of a gas, for example, is attributed to the very small size and irregularity of the motions, which within any region of sensible size are the same, *on the average*, as within any neighbouring region. Within regions of molecular dimensions the distribution of motion is extremely irregular; neighbouring molecules are not in general moving at the same speed or possibly vibrating in the same way. Hence in this view the size at which heat movements are manifested in matter are of molecular sizes, *i.e.* from 10^{-7} to 10^{-8} cm.

In addition, however, to all this matter, motion and vibration, there are present ether motions of an irregular kind. Within any closed envelope at constant temperature the ether motions must be in statistical equilibrium with the motions in the envelope. The energy per c.c. of these ether motions will be considerable at high temperatures, and small at low ones. Many years ago I called attention to the energy per c.c. required in order to, in this sense, warm up ether, and showed that it was quite comparable with that required to warm up a rare gas. Now the size at which these motions are developed are comparable with the wave-length of the ether disturbances of the period of the motion. In the case of irregular disturbances, such as cause temperature phenomena in solids and liquids, one cannot define precisely the size of the ether disturbances, because they are of all sorts of sizes, being irregular in the same way as the matter motions are irregular; but it is known that at each temperature there is a particular wave-length, round which the ether vibrations may be grouped, and that this average wave-length is shorter the higher the temperature. The wave-lengths of these vibrations, so far as they have been observed, vary from $2 \cdot 4 \times 10^{-3}$ to 10^{-5} cm.

It is at once evident that the average size of the ether motions is *very* much greater—quite a thousand times greater—than the size of the molecular motions. In the molecular motions we could not expect to find any irregularity of distribution within distances such as we can see with a microscope, because within a visible volume there will be millions of molecules, and the average motion will be all that we can expect to see. Is it necessarily so as regards the ether motions which exist on so much a greater scale? Is there any way in which these very much larger scale phenomena may be expected to affect matter on a scale comparable with their own size, and which consequently we might expect to be able to see, and which might produce effects on masses of matter consisting of millions of molecules?

We may consider in this connection an analogy from sound. In sound we can have small solid objects, such as masses supported by springs, which give out waves in air very much longer than the sounding object. A tuning-fork, for example, may be only a few centimetres long, and may give out waves a metre long. The balance wheel in a watch vibrating quarter seconds would generate waves—feeble ones no doubt, but still waves 80 metres long, or some 8000 times the linear dimensions of the vibrating object. Similarly the vibrations of molecules generate ether waves many thousands of times as long as the linear dimensions of the molecules. On the other hand, solid bodies may be much longer than the air waves they produce. A bar of steel vibrating longitudinally would be fifteen times as long as some of the air waves it would generate. A pipe full of air would be of about the same length as the air waves to which it would resonate, not less than about a quarter as long. If, then, we had a large number of sounding bodies, some small ones like small tuning-forks, balance wheels, and such like, and others like pipes of a size comparable with the air waves, we would expect that when the small ones were all sounding, the larger ones would resonate to their corresponding waves, and thus be set in vibration by the waves originated in the smaller bodies.

In the case of electromagnetic waves we should expect the same result. If there are bodies comparable in size with the heat waves in the ether which can have electromagnetic vibrations produced in them of the same periods as those emitted by the molecules, these bodies should resonate to these heat waves. Now, by utilising the ordinary process of conduction in metals, we know that it is possible for electromagnetic vibrations to exist in conductors of a small size, down to a few millimetres in diameter; and there is no reason to doubt that by means of conduction very much smaller bodies can have electromagnetic vibrations in them. Dr. Lodge, indeed, has suggested that the structures in the retina are of about the right size to resonate electromagnetically to waves of the frequency of the light waves that affect our eyes. Larger objects would resonate to the electromagnetic vibrations corresponding to the ordinary air temperatures. A sphere 10^{-3} cm. in diameter would, for instance, resonate to waves of about the greatest length that have been measured by Rübens and Nicholls, and a much larger one could have a harmonic of its fundamental tone excited in it by these waves. In addition to these vibrations in conductors, non-conductors of one specific induction capacity immersed in a medium of a different specific inductive capacity could also have syntonic vibrations excited in them. From all this it seems quite certain that in small particles of matter there must exist, at all temperatures, electromagnetic vibrations of a size comparable with the wave-lengths existing in the surrounding ether.

What sort of effects might we expect to be produced by these electromagnetic vibrations? Is there any prospect of our being able to detect them? What amount of energy may there be in this form of vibration on each particle? These are questions to which I am afraid I can only give very vague answers. To the first question, as to what effects may be expected to be produced by these vibrations, I can only suggest in the first place an unequal heating of the particle. The parts of the particle which are the electric nodes, where the electric current alternates and where there are no electric charges, these parts should be kept at a slightly higher temperature than the electric loops. If the particle were not perfectly symmetrical, this would lead to an unequal heating of the particle as a whole, and this may be a cause of those so-called Brownian motions of small particles immersed in a liquid which are so very difficult to explain. In the second place, it may lead to a grouping together of molecules into masses of a size depending on the temperature of the liquid, and to a going about of these groups of molecules and a similarity of the vibrations of the component molecules which complicates the theory of temperature in a way that may ultimately, as I have before now pointed out, explain to some extent the difficulties at present surrounding this theory. In the third place, this may be connected with the conditions for the breaking down of simple viscous motion and the production of vortices in a liquid, though I hardly think an explanation on these lines is required; and, finally, it may be connected with crystalline forces, the structures in the eye, vital actions in small cells and on a small scale, as in the patterns on diatoms, and possibly with the temperature at which vital actions of certain kinds, such, for example, as consciousness, are possible. These are the merest guesses of a wild kind as to the possible results of what seems to be a *vera causa* for structures and actions in matter of a size comparable with the wave-lengths of light, and must be taken as merely wild guesses.

As to the second question, of the prospect of detecting these electromagnetic vibrations in particles of matter, its answer depends so entirely upon the first that I can only leave it to the investigators of the future to try and detect them. That such electromagnetic vibrations exist, I think, can hardly admit of doubt, any more than that the strings of a piano are kept in vibration when loud and irregular noises are produced in its neighbourhood.

As to the energy of the vibrations upon each particle, I cannot give any satisfactory answer. If the particle were in a region through which a series of plane waves of a constant type were being transmitted, it would no doubt be possible to solve the problem of determining the amplitude of its vibrations in particular cases of assumed shapes of particles. In the actual case of irregular disturbances I do not see, at present, any direct way of attacking the problem. It would apparently require to be attacked statistically, but I doubt whether this would lead to a true result, because there seems some reason to think that trains of uniform waves of considerable length do exist in the ether, and if there is any regularity of this kind in the ether motions, a purely statistical treatment, in which the vibrations were assumed to be quite irregular, would fail to lead to a true result. If the energy of these electromagnetic vibrations of its fundamental period on a particle is no greater than corresponds to its one degree of freedom on Boltzmann's theory of partition of energy, I am afraid the amount of energy of this kind on each particle is hardly sufficient to account for any observable phenomenon. That it may, however, be much greater seems justified by the failure of this theory, so far as is known, in other cases, and this must be my excuse for calling attention to what seems certainly a *vera causa* for structures and actions in matter of a size comparable with the heat vibrations in the ether, even though the amount of this cause may, when fully investigated turn out to be so small as to be insufficient to produce observable effects.

GEO. FRAS. FITZGERALD.

REPORT OF THE MALARIA EXPEDITION TO SIERRA LEONE.

FOLLOWING close on the "Instructions for the Prevention of Malaria," the Liverpool School of Tropical Medicine have issued the report of the malaria expedition sent out to Sierra Leone by that body in August last. Their objects, as stated in the report, were :—

(1) To find one or more species of insects hospitable to the human Hæmamœbidæ on the West Coast of Africa.

(2) To study the bionomics of these insects, with a view to suggesting better modes of prevention of malarial fever than those hitherto known to us.

The terminology adopted is that used by Major Ross in consultation with Prof. Herdman, already noticed in NATURE (August 3, 1899). It is proposed to abolish the word mosquito, and use the old English equivalent, gnat, as there is no difference between the two, and because the terms malaria and malarial fever no longer hold—they propose the term hæmamœbiasis, or gnat fever.

The genus Anopheles was chiefly looked for, because these had been shown to be concerned in the transference of the parasite. In the barracks at Wilberforce, Sierra Leone, 25 per cent. of the soldiers suffered from all three forms of malaria or gnat fever. All the gnats caught in the barracks were *Anopheles costalis* except one, and out of 109 of those examined, parasites were found in 27.

Some experiments on feeding Anopheles on a patient with *H. malariae* gave positive results, several young zygotes being found in the gnat. These gnats were caught in a building where there were no fever patients, and numbers of them had been examined and found free from parasites. When, however, Anopheles bred from the larvæ and kept in test tubes were applied to the skin of a patient, they were found not to feed copiously, and negative results, as regards zygotes, were obtained on dissecting them. It is suggested that the explanation of the failure was the non-fertilisation of the females ; it seems that the female gnat requires blood for the nutrition of the eggs. If the ova are not fertilised, the blood is possibly evacuated without some digestive process being performed which may be necessary to the vitality of the zygotes.

Measures of precaution against the bites of gnats, and measures for reducing their numbers, are discussed in the chapter on prevention. It is remarked that neither Europeans nor natives made any effort to keep down the numbers of gnats, which constitute a very serious pest in Sierra Leone, as they do in all tropical towns. Both this report and the "Instructions for the Prevention of Malaria" should be invaluable to residents abroad, as indicating how they may protect themselves from the annoyance from gnats, and from the evil results that may arise from their "bites."

Experiments were instituted with a view to destroying the adults or larvæ, and to prevent the insects from breeding. It was not always possible to discover the breeding pools of the Anopheles infecting a particular spot ; for instance, none could be found at Wilberforce, the nearest pools where larvæ were found being nearly a mile away. Dr. Fielding Ould tried experiments with tar, and found the film on the surface of the pool lasted longer than a film of kerosene oil ; while both killed the larvæ and prevented them from hatching so long as the film lasted.

In the addenda are some good micro-photographs of both zygotes and blasts from the gnat.

JOSEPH BERTRAND.

AMONG the heavy losses which science has suffered during the past few months, few will be the subject of such universal regret as the death, on April 3, of M. Joseph Bertrand. The loss will be felt, not only by mathematicians, but also by the great body of scientific men with whom Bertrand was brought into contact in his capacity of life-secretary of the Paris Academy of Sciences.

Joseph Bertrand was born at Paris in 1823, and at an early age commenced his mathematical studies under the guidance of his father, who had been a pupil of the Ecole Polytechnique. Subsequently Bertrand entered the Collège de St. Louis, and at the age of eleven he succeeded in passing the examination for entrance into the École Polytechnique, although it was not till six years later that he actually entered the college, when he headed the list of candidates. As a boy, Bertrand would nowadays be styled an "infant prodigy," by analogy with the youthful musicians who created such a *furore* at London concerts a few years ago ; and it is interesting to learn from M. Maurice Lévy that this title (*enfant prodige*) was actually bestowed on him by the scientific men who welcomed Joseph as a young colleague at an early stage of his existence. The analogy between music and mathematics seems, moreover, to have suggested itself to M. Jules Lemaitre, Director of the French Academy, who remarks that such precocity of genius is sometimes found in mathematics and in music, but is never seen in literature. We find Bertrand publishing a paper on the theory of electricity in 1839, when he must have been about sixteen years old, and it is hardly surprising in view of this to learn that his precocity amazed his masters. In 1841 he wrote papers on indeterminate forms, Jacobi's theorem and differential equations, and from that time onward he was fairly launched on his career as a writer of mathematical papers, his output being five papers in 1842 and seven in 1843. But whereas most of the young musical *débutants*, to whom reference has just been made, have enjoyed only ephemeral reputations, and have exhausted their energies in their premature efforts to an extent which must have prejudiced their future careers, Bertrand succeeded in achieving all that was predicted of him ; he showed no diminution of energy in advancing years, and, moreover, to judge from all accounts, he developed into a good man of business, a quality which is commonly regarded by "the general public" as incompatible with being a genius.

In 1842 he had a narrow escape from being killed in a railway accident near Meudon. In company with his brother, Alexandre Bertrand, now distinguished as an archæologist, he had gone to Versailles to see the fountains, and on the return journey the accident occurred in which Admiral Dumont d'Erville was killed. Both of the Bertrand brothers suffered, Joseph losing the bridge of his nose—a misfortune which disfigured him for life—while Alexandre's leg was fractured. Joseph rescued his brother by dragging him through "the skylight," the carriage doors being locked. A few months later he